Response to Reviewer 1

Julia Menken

April 18, 2026

Main comments

The authors present an interesting statistical analysis of utility-scale turbine wakes under different and carefully characterized atmospheric conditions. The work is relevant to improving the understanding of wake physics and wind farm modeling and adds some novelty to existing literature. A few minor revisions are recommended to improve readability and rigor.

Thank you for your constructive feedback and helpful suggestions which have improved our manuscript. We particularly appreciate the thoughtful comments on lidar processing and accuracy. In the following, we address the main and specific comments and describe the corresponding modifications to the manuscript.

In Section 3.2, it may be beneficial to add a – in front of A, so that A becomes a positive-definite constant. This would avoid awkward definitions like "the amplitude A is greater than -0.1U" to indicate a shallow wake.

We agree that changing the sign of A adds to comprehension here. We have changed the sign in Eq. 5-7 and adapted the usage in the further text, i.e. for the wake detection termination criterions *"the amplitude A is greater than 0.1U"* and the definition of ΔA *"if $\mu_1 < \mu_2$, then $\Delta A = A_2 - A_1$ otherwise $\Delta A = A_1 - A_2$"*.

In Section 3.1, the lidar filtering process should be expanded and cite previous work (e.g., Beck and Kuhn, 2017).

We added some more explanation and details on the filtering steps, especially on the two-dimensional median filter, we applied. For this filtering, we mainly followed the ideas of Alcayaga, 2020. For a more complete categorisation of the filtering, we also add Beck and Kuhn (2017) to the references.

The possibility that the lidar could have an azimuth offset with respect to the turbine axis should be mentioned. Alternatively, the pointing accuracy of the lidar should be quantified. This is important to ensure that the wake deflection (which is on average 30 m at 4 D or 3.7 degrees) is real. Also, for the asymmetric wake model where the center is a weighted average between the two peak locations, the asymmetry may just induce an apparent deflection if one side of the wake is consistently deeper. This should be discussed as well.

We agree that the wake deflection estimate is susceptible for azimuth offsets of the lidar. Mechanically, the lidar is aligned to the nacelle with very little play in the screwed connection. The optics of the lidar and scanner head inaccuracies can of course lead to offsets. Vasiljevic (2014) determined the pointing accuracy for the scanning head type which is used in our lidar to approximately 0.5° without any further correction, wherein the largest part of the uncertainty is due to the leveling of the lidar. To minimize the error, we performed hard target scans, scanning towards the meteorological masts at the research park after installation of the lidar (see example hard target scan, Fig. 1). The biggest uncertainty in a determination of the pointing direction of the lidar in the wind turbine setup is the uncertainty of the yaw angle of the turbine. With the available sensors

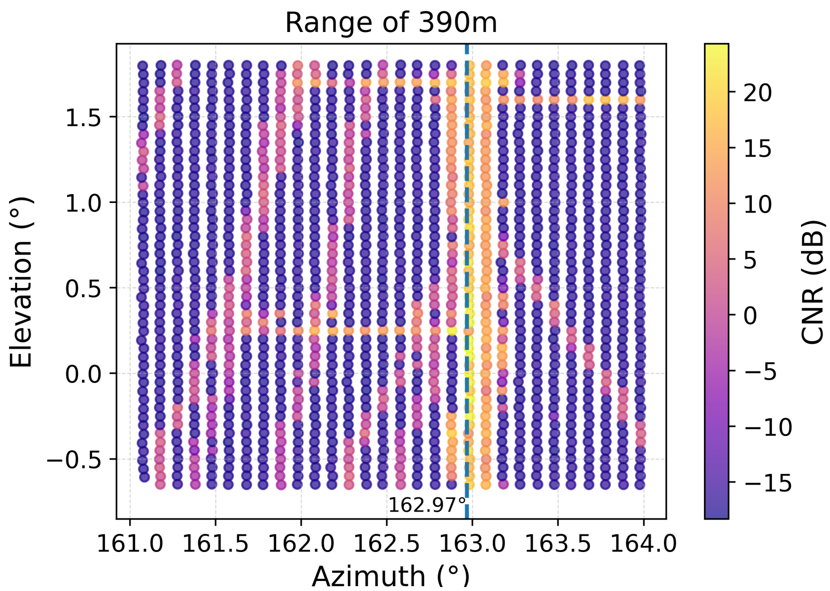

Figure 1: Hard target scan with azimuth and elevation resolution 0.1°. Based on yaw angle of the turbine calculated position of the measurement mast at 162.97° (dashed blue line). High CNR values (CNR≥10 dB) illustrate the mast structure itself at 163° azimuth with its booms and slightly lower CNR values (10 dB≥ CNR≥-5 dB) highlight the guy wires.

at time the dataset was taken, this could only be achieved within approximately 1°. In response to your review, we have now carefully revisited the pointing accuracy during the time frame of the presented dataset using hard target reflections of the masts during operation. This additional analysis helped to implement an average offset which was estimated to 0.3° to the dataset which should further improve the estimates of wake deflection and yaw misalignment (see Fig. 2). It also gives a good estimate of the uncertainty of the pointing direction of the order of 1° which we now mention in the revised manuscript. At $4D$ downstream, such an uncertainty yields an uncertainty of the wake deflection of 8 m, which is substantial, but much smaller than the wake deflection itself, which we thus consider a physical effect and not an artifact of the azimuth offset.

By using the weighted average for the asymmetric wakes, the wake center refers to the centroid of the distribution. Therefore, it is reflecting the asymmetry of the intensity of the wake. We thus agree that parts of the wake deflection are associated to the asymmetry effect and discuss it in the revised manuscript in Sect. 3.2 and 5.1. Further, the impact of asymmetry on wake deflection decreases with downstream distance as the wake resembles a single Gaussian. This is demonstrated in the frequency of best fits to double Gaussian with two amplitudes which is decreasing from 67% of cases at 1 D, 32% at 2 D, 19% at 3 D to 17% at 4 D.

From a more pragmatic standpoint, it should be acknowledged that the near-wake physics, in spite of its scientific interest, is less of a concern for industrial applications, where turbines are spaced significantly further along the prevailing wind directions.

We agree that most spacings between wind turbines in wind farms are further than the near wake distance. However, the industrial interest in the investigation of near wake dynamics and length is at least two-fold: on the one hand, repowering existing onshore wind farms leads to closer relative spacing. Especially in secondary wind directions this does indeed lead to spacings below the minimum defined distance for load calculations. In this case, sector-wise shutdowns have to

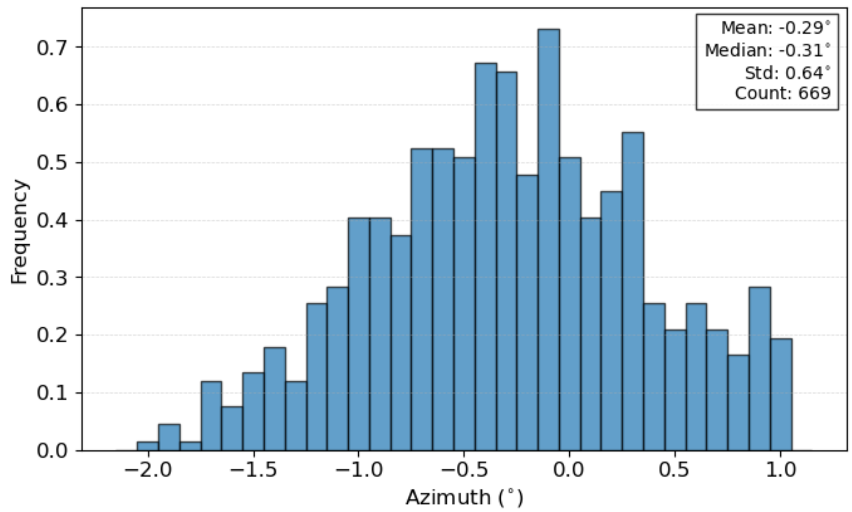

Figure 2: Relative frequency of offset in azimuth angle. The offset is given by the maximum of CNR around the position of the middle measurement mast. The data covers six hours of single lidar scans spread throughout six different time periods from Nov 2023 to June 2024.

be implemented, which has a significant impact on wind farm performance. On the other hand, it has recently been shown that engineering models for the far wake are significantly influenced by a virtual origin that is used in the models and is related to the near wake length Neunaber et al. (2024). That means that engineering models can profit directly from better estimates of near wake length. In the revised manuscript, we will explain these perspectives in the conclusions more explicitly.

Specific comments

- L 28: Please clarify that these are canonical atmospheric conditions and that there are many exceptions (for instance, during low-level jets).

  Thank you for suggesting clarification here. We edited the manuscript to now include *"ABL stability is often described using canonical regimes, although real-world ABL conditions frequently deviate from this idealized state, e.g. during low-level jets or frontal passages."*

- Eq. 3: This approach only includes the part of induction that is converted to power, which will probably cause an underestimation of thrust. Drag from tower, hub, and blade roots all contribute to thrust but not power. Electrical losses cause an underestimation of thrust. Please clarify this point and possibly cite Iungo et al., 2018.

  We appreciate this comment and have revised the manuscript to clarify the limitations of this approach. The thrust estimation solely based on power indeed neglects contributions from non aerodynamic components. We have added more explanation on these effects along Eq. 3 as follows: *"Here, the calculation of c_T relies on the induction factor derived from power extraction and does not account for electrical losses or drag from components not contributing to power production, such as the tower, nacelle and blade roots. Consequently, c_T may be lower than the turbine's actual aerodynamic loading (Iungo et al., 2018)."*

- L 113: Please explain the implications of measuring inflow quantities at 85 m which is less than hub height.

  Thank you for commenting on this. In the revised manuscript we now give more details on deviations resulting from using 85 m instead of hub height values in Section 2.1. *"Although the height difference of 7 m between the sonic at 85 m and hub height introduces minor discrepancies, the observed wind shear values (mean: 0.029 s^{-1}, max: 0.086 /s) suggest that the differences in wind speed are small, on the order of 0.203 ms^{-1} on average and up to 0.602 ms^{-1} at maximum shear. Similar considerations apply to wind direction, where the mean difference is 0.371° and the maximum difference is 3.465° based on observed veer between 33 m and 149 m."*

- L 146: "The scan ends" is ambiguous, you can say that "the beam are blocked by the ground at 700 m".

  Thank you for pointing that out. We have revised the manuscript to clearly state that the scan range is limited to 700m due to blockage of the beam by the ground.

- L 149: Please specify the duration of the 6,099 periods (27 s? 10 minutes?).

  We thank for asking for clarification here. We refer to the periods here as 10-minute periods in the whole paragraph and adapted this in the text.

- L 152: How are "free-stream atmospheric conditions" defined?

  Thank you for this question. We have changed the terminology to use *"inflow measurements at 2 D upstream"* instead.

- L 170: Please give more details on the data filtering, namely the way the "neighborhood" is defined and the "threshold" for the median deviation.

  As stated in the General comments, we have added more details about the filtering procedure including the description of the "neighborhood", i.e. in total 11 range gates and 5 azimuth angles. The thresholds are set to two standard deviations for CNR and three standard deviations for radial velocities. We have adapted the text as follows: *"For filtering lidar data to improve data quality, several methods have been proposed, e.g. by Krishnamurthy et al. (2012), Beck and Kühn (2017) or Alcayaga (2020). To reduce noisy and erroneous measurements, the nacelle lidar data is filtered to retain only data with carrier-to-noise ratio (CNR) values between -25 and 5 dB. For enhanced robustness, a median filter, similar to Alcayaga (2020) who used a median-like filter on radial velocities, is applied to each individual scan to first CNR values and second radial velocities. Instead of using two one-dimensional moving windows like Alcayaga (2020), we apply two-dimensional windows considering radial and azimuth direction at once. This filter evaluates each data point by computing the median of CNR or radial-velocity values within its local two-dimensional window, which encompasses 11 points in radial direction and 5 points in azimuth direction. Measurements that deviate beyond a predefined threshold from the local median are excluded as outliers. Specifically, for the CNR median filter, the threshold is set to two standard deviations of the CNR values of the scan, while for the radial velocities, the threshold is three standard deviations of the radial velocities of the scan. These thresholds were determined through manual optimization on using a test set of scans, effectively removing hard targets and artifacts while preserving the wind field structures, including wind turbine wakes."*

- L 205: Please provide a reference for the F-test.

We have added a reference to Aitken et al. (2014) to support the application of the F-test.

- L 237: Can this be said more concisely as "when the difference between the centerline and maximum velocity deficit is less than 5%"?

  We agree that the phrasing was unnecessary complicated. We changed the description in the updated manuscript to make it more concise and clear to state: *"Therefore, we introduce an additional criterion for determining the near wake length: the transition occurs where the difference between the wake center and the minimum velocity ratio is less than 5%."*

- Fig. 7: The μ symbol should be introduced earlier. Also, please expand on the effect of the second wake past 4D and how it is smoothed out in these statistics by the quite large wind sector.

  Thank you for this helpful suggestion. The parameter μ has now been introduced earlier, when describing the Gaussian functions and how it is calculated for the different Gaussian forms in Sect. 3.2. Additionally, we have expanded the discussion on Fig. 7 to clarify the smoothing of the OPUS2 wake impact as we are using a wide wind direction sector as follows: *"At this distance, the wakes of the two turbines start to overlap. Though, OPUS2 is not located in the wake of OPUS1 in all cases due to the width of the chosen wind direction sector, i.e. the wakes of the two turbines will not always overlap. Consequently, the effect of OPUS2 wakes on the wake characteristics seen from OPUS1, e.g. on the velocity deficit, is smoothed out. Further downstream of OPUS2 the velocity deficit weakens again."*

- L 273: "The induction zone of the second turbine impacts the wake dynamics of the first turbine additionally" should be removed. There is no wake dynamics provided in this section but only mean quantities. Furthermore, the induction zone is probably too thin for the present lidar and grid resolution to isolate its effect.

  We appreciate this feedback. We removed the statement about the impact of the induction zone of OPUS2. We agree that the lidar resolution may not be fine enough to separate the induction effects, thus we did not discuss it further in the revised version.

- L 302: Please add discussion also of the wake width.

  Thank you for mentioning this. We expanded the discussion to include the correlation of wake width with turbine operating conditions as well which states: *"The wake width exhibits similar correlation patterns as the velocity deficit. σ_w is highest correlated with wind speed and turbine conditions c_T, c_P and λ at 1 D with these correlations diminishing further downstream. The negative correlation between σ_w and U can be explained by the significant reduction in wake width above rated wind speed. Therefore, the positive correlations with c_T, c_P and λ are predominantly caused by the fact that these parameters, like σ_w, decrease above rated power."*

- Fig. 11: Please explain better the legend either in the caption or the section.

  We agree that the figure description lacked some details about the legend. We have now added more explanation in the figure caption (*"Histogram of detected near wake lengths in (a) with stacked bars which colors indicate the choice of the criterion for near wake length determination"*) and referred to the colors of the bars in the text as well for clarification.

- L 415: "wake tilt" may be mistaken for vertical deflection, while it sounds here we are talking about the skewness or stretching of the velocity deficit in the cross-stream plane. Please clarify.

Thank you for this suggestion. We agree that the term "wake tilt" could be ambiguous. Therefore, we have revised the manuscript to describe it as *"skewness in the cross-stream plane"*.

- L 424: Please add a reference to a simple wake model that uses turbulence to predict wake deflection.

  We appreciate the reviewer's comment and we acknowledge a mistake in our earlier statement. We claimed that there are simple wake models taking turbulence into account to estimate wake deflection. While turbulence is often considered for modeling wake expansion, we haven't found a simple model that uses turbulence for estimating wake deflection. We have corrected the sentence to solely mention yaw misalignment and removed turbulence from this statement.

- L 520: The link between (tip?) vortex dynamics and wake asymmetry is unclear. Earlier it was claimed that the asymmetry arises from a combination of wake rotation and shear, with stability enhancing its persistence. Please resolve this ambiguity.

  Thank you for this valuable comment that actually touches on an interesting question that our findings raise. We see that there is an asymmetry in the mean values of the wake parameters, as well as in the occurrence, circulation and core radius of tip vortices (Wildmann and Kistner, 2025). At this point, we cannot clearly disentangle the causality between the dynamics in the near wake. Parameter studies in LES suggest that the asymmetry is clearly related to the inflow shear and veer in combination with the turbine rotational direction, but if tip vortex stability is enhancing this effect, or vortex breakdown is more effectively forced by stronger shear on one side of the wake remains to be shown in future studies. We will thus change the statement about tip vortices in the manuscript, simply stating that the *"interaction between tip vortices and the asymmetric mean flow requires further studies to fully understand the near wake dynamics."*

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Response to Reviewer 2

Julia Menken

April 18, 2026

General comments

The paper "Impact of atmospheric stability and turbulence on wind turbine wake characteristics: a nacelle lidar study" provides a detailed analysis of the influence of atmospheric conditions on near wakes compared to turbine operational parameters. It emphasises the importance of considering atmospheric conditions for near wake models. The nacelle lidar dataset covers an extensive period from November 2023 until June 2024. The paper is generally well-written and the topic is relevant to the scientific community. To improve the manuscript further, I would suggest minor revisions.

Thank you very much for your thoughtful and constructive feedback on our manuscript. Below, we address your specific and technical comments and indicate the corresponding changes we have made to the manuscript. We appreciate that you took the time to read our manuscript and for sharing your expertise, which has contributed to improving our study.

Specific comments

- L20 ff: Please add citations for the statements you are making, e.g. on the topics: increased load on downwind turbines due to the wake, wake models for layout optimization, low complexity of models, real world conditions are more complex → see near wake region and atmospheric inflow conditions.

  Thank you for this suggestion. We have added relevant references, the manuscript is adapted as follows: *"The wake of a wind turbine is a region characterized by reduced wind speed and increased turbulence that forms behind the rotor as the wind turbine extracts energy from the flow (Porté-Agel et al., 2019). Wind turbine wakes impact the efficiency of wind farms by not only reducing energy production due to lower wind speeds in the farm (Barthelmie et al., 2009; El-Asha et al., 2017) but also by increasing the loads on downwind turbines, which can lead to fatigue and a reduced lifespan (Thomsen and Sørensen, 1999; Kim et al., 2015). To mitigate these effects, research has focused on understanding the wake dynamics and integrating the knowledge into wake models which serve as useful tools for layout optimization, wind resource assessment and wind farm control (Archer et al., 2018; Fleming et al., 2014; Pedersen and Larsen, 2020). To maintain computational efficiency, these models are often of low order complexity (Amiri et al., 2024). However, this leads to a simplification of complex real-world characteristics, including the near wake region and the variability of atmospheric inflow conditions (Abkar and Porté-Agel, 2015; Meyers et al., 2022)."*

- L. 146 Can you please specify how you calculated the intersection with the ground, because if you assume that the nacelle lidar is located at 92m, then the intersection of the beam with the ground would be around 755m not 700m.

You are right. Our initial estimate was indeed rough as it was based on the location of high CNR values in the lidar scans. Using the nacelle height of 92 m and 7° elevation angle, the ground intersection occurs at 92 m/ $\arcsin 7°$=751 m, as now stated in the revised manuscript in Section 2.2. Nevertheless, in the lidar scans disturbances may appear already at a closer distance due to the width of the lidar pulse (FWHM of 50 m at 200 *ns* pulse width). Hard targets (like the ground) at the pulse edges can cause stronger backscatter than aerosol in the center, explaining the observed interference starting at around 700 m.

- L. 147: Why does it say data until December 2024 in this line, when the abstract states that the data analyzed is available until June 2024.

  Thank you for mentioning this point. We appreciate the opportunity to clarify this. While the lidar measurements have been conducted continuously from November 2024 and are still ongoing, our study is limited to data up to June 2024 due to consistency of available inflow mast data (required for veer, shear and stability quantities) and reliable operation of the lidar, which was subject to some failures in later periods and was replaced in 2025. Due to the responses to a comment of Reviewer 1, the number of cases has changed slightly from the original manuscript. We have included more details in the revised manuscript (Sect. 2.2) on the number of 10-minute periods retained after each filtering step: full dataset: 19804, wind direction sector: 6171, normal turbine operation: 4964, and availability of inflow data: 1396.

- L. 150: Could you please specify this further? Because in L. 287 you say that the wake of OPUS1 is influenced by the wake of OPUS2 at 4D downwind.

  Thank you for pointing this out. We have rephrased the passage to state that OPUS1 is not operating in the wake of OPUS2, but indeed there will be wake interaction with OPUS2 wake starting at 4D downstream of OPUS1 for the selected wind direction sector, as follows: *"Additionally, it guarantees that OPUS1 is not operating within the wake of OPUS2. While OPUS2 is located downstream of OPUS1 under these conditions, wake interactions begin at 4.3 D, which is not relevant to this study as it focuses on characterizing the wake of OPUS1 up to 4 D downstream, as measured by the nacelle lidar."*

- L. 186: Can you explain why you introduced VD when you did not use it and only used v_h/U in figure 4. VD is then reintroduced in L. 267 and used in figure 7.

  You are correct. We introduced VD but didn't refer to it until Section 4 as we were more focused on showing the velocity ratio (v_h/U). We have moved its definition to Section 4 in the revised manuscript to avoid confusion (see also later comment on Fig. 7).

- Fig. 4: This figure is very interesting and displays a lot. It would be helpful to add more references of the figure to the text and maybe also some more explanation as e.g. why you used this day during nighttime.

  The example in Figure 4 is now explained in more detail in the text in Section 3 after the wake detection algorithm was introduced. This example shows a quite long wake where the different development stages of a wake are visible and also the usage of the different Gaussian functions is highlighted. We choose this wind direction as the wakes of OPUS1 and OPUS2 do not overlap which allows to interpret solely the wake of OPUS1 far downstream. The manuscript in Sect. 3 now states *"An example of a processed horizontal PPI scan is shown in Fig. 4b. A night-time case with stable conditions is selected where the wake's velocity deficit is strongly pronounced and it is persisting far downstream, even beyond 10 D. Lateral cross-sections (Fig. 4c-f) demonstrate the evolution from resembling a double Gaussian with* *asymmetric amplitudes, over a double Gaussian towards a single Gaussian in the far wake. Complementary, Fig. 4a indicates the best fitting Gaussian function. The wake centerline moves slightly away from the turbine hub towards $y < 0$ downstream. Also, in Fig. 4b the filtering of the scans excluded spurious measurements around $(x, y) = (3D, 2D)$, where the three masts and OPUS2 are located."*

- L. 213ff: This section describes wake characteristics but changes metric a few times between meters and diameter. I would suggest being more consistent with that and when e.g. talking about the wake length to always refer to the diameter. (also L. 275)

  Thank you for this helpful comment. We agree that consistency is important. In L. 213ff, we choose to keep using meters for lidar related specification of the wake detection algorithm, since the lidar data is on a 10x10m grid. This applies also for example for the first measurement distance later on which is given in meters as well. Otherwise, we refer to rotor diameters with variables concerning the turbine wake including e.g. wake width.

- Fig. 5: To stay consistent, I would suggest to also add "D" to the y axis values, as e.g. in figure 4.

  We appreciate this thoughtful suggestion. We have updated the figure to extend from - 0.5 D to 0.5 D on the y-axis, thus we changed the parameters slightly. As it is an analytical example, the message remains the same, though readability and interpretability of this figure are enhanced and better align with the dimensions of a turbine wake.

- L. 237: Could you please rephrase the sentence describing the new criterion? The term velocity ratio should also be introduced and defined.

  Thank you for pointing this out. We agree that the phrasing was unnecessary complicated. We changed the description in the updated manuscript and introduced velocity ratio accordingly as follows: *"Therefore, we introduce an additional criterion for determining the near wake length: the transition occurs where the difference between the wake center and the minimum velocity ratio is less than 5%."*

- L.244: Could you please add the meaning of the abbreviation RHI?

  Thank you for catching this. We have now introduced the full term, range height indicator.

- Sec. 4: I would suggest to add 1-2 sentences about the location of the measurements you used for the meteorological properties or refer to the section where you described that.

  You are correct. We haven't introduced these measurements in the results section very well. We have now added additional information when introducing what kind of data is used in the analysis and are referring to Sec. 2.1 in the revised manuscript. We have adapted the text as follows: *"The analysis includes wake data of OPUS1 derived from 10 minute averaged lidar scans, measured as described in Sect. 2.2, as well as turbine operational parameters of OPUS1 and meteorological parameters, provided by the inflow mast located west of OPUS1, the latter two are described in Sect. 2.1."*

- L. 272: It is a little confusing that you say the location of OPUS 2 is at around 4D when in figure 7 the captions says that the line is at 4.3D.

  Thank you for mentioning this ambiguity. We adapted the text to use 4.3D as well as this is the more precise location.

- L. 278: Can you explain how μ was calculated? What wind data did you use?

  Thank you for mentioning the lack of clarity. μ represents the wake center in cross-wise direction at hub height. We introduced it in the updated manuscript in Sec. 3.2 as it is a derived quantity from the Gaussian fits. We have renamed the variables in Eq. 5-7 from b, b_1 and b_2 to μ, μ_1 and μ_2 respectively to keep the naming convention clearer. We did not use other wind data then the nacelle lidar data to calculate the wake center.

- Fig. 7: How is the velocity deficit calculated? Did you compare the minimum in the wake to the wind speed measured by the nacelle lidar outside of the wake or to the tower measurements? What uncertainties do you induce by comparing different altitudes (nacelle at 92m; sonic at 85m) and different measurement principles (lidar and sonic). And how is the influence of the beams being outside of the rotor plane above 4D downwind of the wind turbine? I would also suggest emphasizing in figure 7a, b, c that the data is not consistent, because the lidar has a resolution of 20m.

  Unfortunately, it seems we did not make the calculation of the velocity deficit clear enough. We used the wind speed of the inflow measurement mast at 85m U and the minimum wind speed in the wake U_{min} as basis, thus VD is calculated through $1 - U_{min}/U$. In the revised manuscript we adjusted the explanation in the beginning of Section 4.1 as follows *"Fig. 7 shows the mean values and standard deviations of the velocity deficits VD, which is defined as $1 - U_{min}/U$ with U_{min} the minimum velocity in the wake region (wake center $\pm\sigma_w/2$),..."*. We give more details in the revised manuscript on the implications using data from the sonic at 85m in Sect 2.1. In detail, we state *"Although the height difference of 7 m between the sonic at 85 m and hub height introduces minor discrepancies, the observed wind shear values (mean: 0.029 s^{-1}, max: 0.086 /s) suggest that the differences in wind speed are small, on the order of 0.203 ms^{-1} on average and up to 0.602 ms^{-1} at maximum shear. Similar considerations apply to wind direction, where the mean difference is 0.371° and the maximum difference is 3.465° based on observed veer between 33 m and 149 m."*.
  Lidar and sonic anemometer measurements differ due to the lidar's larger measurement volume, lower temporal resolution and assumptions about wind homogeneity when the data is processed, which can lead to underestimations in velocities especially when the wind field is inhomogeneous with e.g. steep gradients.
  Beyond 4 D downstream, we do not use the +-7 ° elevation scans for vertical information of the wake. This is indeed due to the fact that the beams will be measuring outside of the rotor area (Fig. 2b). Actually, we use the vertical information up to 2 D downstream in our analysis and beyond that we solely analyze the 0 ° elevation scan. Additionally, we mention the resolution of the lidar data in the figure caption of Fig. 7abc in the revised manuscript.

- L. 283 How do you define the "wake deficit", is it the same as VD or is it v_H/U, please specify?

  Thank you for bringing this misleading phrasing to our attention. We changed it to velocity deficit to clarify that it is about the quantity VD.

- L. 300: Can you please specify if wake centre deflection, wake deflection, lateral deflection and wake tilt are the same and maybe only use one of the words throughout the paper to make it easier to follow.

  We appreciate your concerns and agree that the terminology used in the manuscript was unclear. Indeed wake center deflection, wake deflection and lateral deflection mean the same, though wake tilt refers to the vertical slope of the wake centers at different heights. To improve clarity, we have applied the following consistent terminology. "Lateral wake center deflection" is now used to describe the variable μ in the revised text in Sect. 4.1 and generally instances referring to wake center deflection, wake deflection and lateral deflection in the whole script have been updated to this term except for a few occurrences to not lengthen the text unnecessarily and keep it concise, e.g. when in the same paragraph the whole term was used already. Additionally, we have replaced "wake tilt" with "vertical wake skewness" throughout the manuscript, as this more accurately describes the vertical slope of the wake center at different heights.

- Fig. 9: Could you please add another value to the y-axis, that would make it easier to estimate the magnitude of VD?

  If we were to include further details on the axis, this plot would reveal information about the specific turbine control strategy. Therefore, we unfortunately cannot provide these due to confidentiality agreements. The figure in this manuscript is supposed to show the filtered regions qualitatively.

- L. 320: Please further elaborate this. It would help if you would describe the cause more straight forward. "Under stable conditions VD is stronger (also described in literature). The negative correlation between VD and TI and e supports that lower TI and e values are associated with stable conditions."

  Thank you for this suggestion. We have expanded the discussion on the connection between stable ABL, less turbulence and stronger velocity deficits of the wake, as follows: *"Correlations are calculated for meteorological parameters with wake characteristics for this filtered dataset and shown in Fig. 10. The velocity deficit is more pronounced under stable conditions, as indicated by weak positive correlations to Ri and $d\theta/dz$ at 1 D. As shown e.g. in a LES study by Abkar and Porté-Agel (2015), stable stratification suppresses turbulent mixing, leading to stronger, more persistent wakes. In stable ABLs turbulence, here represented by TI and e, is usually lower. Consequently, negative correlations of VD with turbulent characteristics TI and e support that wake velocity deficits are stronger under stable conditions."*

- Sec. 4/5: It would help to define a clear wording when talking about correlation e.g. 0: zero; 0.01-0.3: weak; 0.3-0.7: moderate; 0.7-1: strong. Because strong is used interchangeably for different values and not consistently.

  We appreciate this helpful suggestion. We have followed it and used consistent terminology for correlation strengths throughout the manuscript.

- L. 334: While here you are saying that there is no significant correlation between μ and Ri and the potential temperature gradient, in L. 319 you say that VD is stronger during stable conditions. While the values for Ri and the potential temperature gradient are very similar for VD (at 1D: 0.17; 0.33) and μ (at 1D: -0.18; -0.34) the wording implies there is a significant difference. I would suggest adjusting one of the sentences to be consistent in the wording.

  Thank you for catching this. Due to the responses to a comment of Reviewer 1, the correlation values have changed slightly, so this sentence had to be revised. Nevertheless, as mentioned in the previous comment, we have now adopted the suggested wording throughout the whole manuscript.

- Sec. 4.3: Something that was missing from my point of view was the explanation on why some correlations differed so greatly between the filtered and the unfiltered dataset: Why is the correlation between Ri and ΔA for the unfiltered dataset as negative but for the filtered dataset weakly positive? Why is the correlation between ΔU and VD negative in the unfiltered and positive in the filtered dataset? What is your explanation to why the slope is independent from filtering?

Thank you for your helpful input on this chapter. We agree that the differences between the unfiltered and filtered datasets were not explained well enough. Therefore, we have included a paragraph in Section 4.3 to provide further information on why the correlations differ. The discrepancies arise from different mechanisms, as follows:

- Correlation between Ri and ΔA: In the unfiltered dataset, negative correlations emerge due to cases of negative lateral asymmetry under strongly stable conditions, i.e. high $d\Theta/dz$, Ri and $\Delta\delta$. These cases occur at low wind speeds and low turbulence. They skew the distribution of ΔA against stability parameters, such as Ri. In the filtered data, these data points are removed, thus revealing the underlying positive correlation with, for example, Ri.
- Correlation between ΔU and VD: In the unfiltered data, low VD above $U_{\max}$ corresponds to high ΔU, pushing the correlation towards negative values. Filtering the data reveals the relationship between increasing ΔU with more stable conditions, leading to positive correlations of ΔU with VD.
- Correlations of slope independent of filtering: Correlations with the vertical wake slope remain consistent across the datasets because turbine operation appears to impact the vertical skewness only minimally. However, veer and stability parameters dominate the extent of the vertical slope, since their effect persists independently of the turbine operating state.

- L. 373 I would suggest to already add a sentence here, even though there is a sentence in L. 464, describing that the first datapoint of the lidar is 100m and therefore the mean needs to be considered with caution because it cuts off everything below 100m.

  Thank you for this valuable suggestion. We have added a sentence after introducing the histogram stating *"However, the distribution is truncated at a minimum near wake length of 110 m, since the lidar measurements begin at 100 m."*

- L. 434: Please specify, if you mean the velocity deficit or if you are referring to v_h/U which is e.g. displayed in figure 5 to illustrate the transition in the near wake region.

  Thank you for pointing out that we have to clarify what we mean here. We changed the phrasing in the revised manuscript to *"The asymmetry of the double Gaussian velocity ratio ..."* to be consistent as we deduced the asymmetry from the Gaussian fits.

- L. 450: The manuscript emphasizes the potential use of this data for modelling and application, is there an opportunity to make this dataset available?

  We appreciate your interest in making the dataset available. While we do not plan to publish the complete dataset of this study, we are planning targeted benchmarks using data from the WiValdi wind farm in the near future.

- L. 507: Even though the dataset covers multiple seasons, how well represented are they in the filtered data? For the analysis you only use 1375 out of 20098 10min periods, is there a pattern, e.g. mostly nighttime, exception in certain month/seasons?

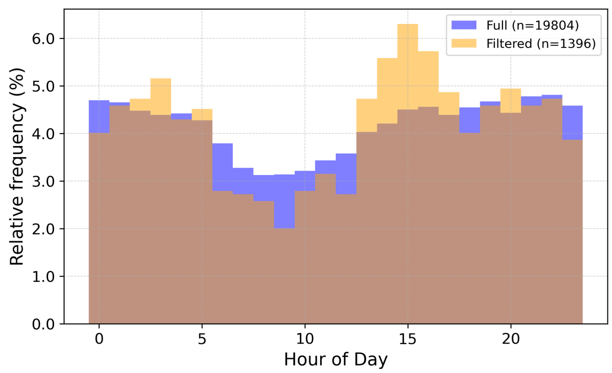

Figure 1: Hour of day distribution for full (19804) and filtered (1396) dataset

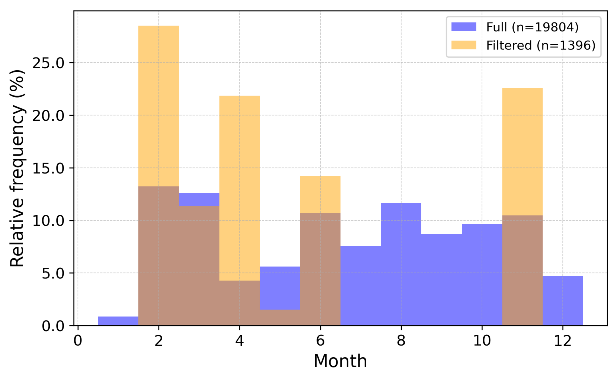

Figure 2: Month distribution for full (19804) and filtered (1396) dataset

Thank you for raising this point. The filtering ensures that only wake measurements with available inflow measurements, including various heights, are retained. The distribution throughout the hours of the day is comparable for the 19804 and 1396 10 minute periods (slightly adapted number of cases see comment to L. 147), see Fig. 1. There is a slight higher representation for cases from 13-15:00 in the filtered dataset compared to the full dataset and lower representation during 5-12:00. However, we consider these deviations to be small enough to treat the subset as representative. Due to the availability of inflow measurements, the dataset ends in June 2024, so we indeed lack data for some summer and autumn months. Additionally, due to turbine maintenance or operation not being in normal power mode, data is unavailable for December 2023 and January and May 2024, see Fig. 2. Nevertheless, the analyzed dataset covers a broad range of atmospheric conditions as shown by the histograms in Fig. 6.

Technical comments

- L. 109: "wind measuring sensor" could be more precisely replaced with sonic anemometer

  Thank you for this suggestion. We have revised the manuscript to specify that the masts are equipped with sonic and cup anemometers and wind vanes.

- L. 128ff: Please be consistent with introducing and referring to parameters with their abbreviations. In this paragraph sometimes there is no abbreviation, sometimes there is only the abbreviation and sometimes there is both.

We have reviewed this passage and ensured that all abbreviations are introduced at the first occurrence. Later mentions use either the long form or the abbreviation.

- L. 158: There is a space missing “wind speed.Additionally”

  Thank you for catching this detail. We have corrected this.

- L. 335: the minus in front of 0.18 is missing

  We appreciate you pointing out this sign mistake. We have changed this.

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